number one hundred and nine died, making, for the temporizing or "let alone" policy, a mortality of 34.7 per cent. These figures demonstrate that œsophagotomy is not resorted to as often as it should be.

The literature of the operation increases in interest as we compare the light in which it was erroneously held as regards danger by those who first performed it, with the actual results of one hundred and forty-five years. By such men as Sir William Fergusson and Nélaton, œsophagotomy was pictured as a dangerous operation. The former states (Cheever's monograph) that, "however simple œsophagotomy may appear on the dead subject, it is attended with much danger on the living, and we should proceed with great caution." Nélaton farther on is quoted as saying, "we must not conceal from ourselves the dangers inherent in this operation, which is certainly one of the gravest and most difficult in surgery." His opinion was doubtless influenced by the rather difficult operation which he advised, viz., the median. By such men as Cheever, Hamilton, Durham (in Holmes's *Surgery*), Bryant, and probably all the standard authors of our day, œsophagotomy is considered neither difficult nor dangerous in its results when performed at the proper time.

FORT RENO, I. T., Feb. 1, 1883.

ARTICLE VIII.

LUPUS AND ITS RELATION TO TUBERCULOSIS.[1]

By ROBERT B. MORISON, M.D., Prof. Dermatology and Syphilis in the Baltimore Polyclinic and Post Graduate Medical School.

THE controversy over the nature of lupus has been enlivened and infused with a new interest since Koch's discovery of the tubercle bacillus. The disease is a comparatively rare one in this country, and therefore its diagnosis from diseases similar to it has frequently not been accurately differentiated. Most authorities agree that syphilis has nothing to do with its etiology, although there may be a coincident syphilitic diathesis.

It is still an open question, and at present a much mooted one, whether lupus is not a local tuberculosis. Friedländer, Volkmann, and many other authorities state decidedly that it is, while Virchow, with many others agreeing, thinks that it is not. Both parties argue from microscopical investigation, and arrive at their respective conclusions in an apparently convincing manner.

If it was possible to decide the nature of diseases with the miscroscope alone, leaving out their clinical study altogether, it would be a simple problem to classify them; but, unfortunately, most of the specific appearances discovered since the perfection of the microscope have been found upon further investigation to be common elsewhere. We have but to call attention to the giant cells once supposed to be peculiar to tuberculosis, and to nothing else, or to the caudated cells of cancer, formerly dominated cancer cells. These do not at the present day rank as distinctive of any diseases, but have found their place among the morbid changes occurring in many.

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[1] Read before the Baltimore Academy of Medicine, February 5th.

Without a specific something known to belong to a certain disease, the microscope does not always admit of a diagnosis. This is especially true of the skin, for the general appearances in it of almost any chronic inflammation are microscopically the same. In considering lupus and tuberculosis of the skin, this fact is not to be forgotten. Microscopically, they cannot be told apart, for even if we do not find caseation in lupus, yet it is often not found in tuberculosis. If we leave out the clinical history then, and trust merely to a microscopic examination of a case, it would be impossible to say decidedly whether it was lupus or tuberculosis.

The study of the two diseases is a curious one. The very men who consider them identical acknowledge their clinical difference, while those who consider them distinct, calling upon these clinical differences to uphold them, have in their turn to acknowledge the great microscopical similarity. Neither party is quite satisfied that the question has been definitely settled.

Koch's discovery of the tubercle bacillus is now being used as a factor in the determination of the nature of these two diseases, and it is with the idea of giving the results of my own investigations as well as those of others, that this paper has been prepared.

The influence which this relationship of lupus to tuberculosis may have upon the validity of Koch's theory cannot be denied. Are we to consider that, if the bacillus described as peculiar to tuberculosis is not found in lupus, it must be classed among the non-tuberculous diseases? On the other hand, if the bacillus is found, would it not be rational to doubt the statement that tubercle is the only disease where such a bacillus is found? In other words, in two diseases which are clinically different, but in which the same microscopical products are found, are we to consider them as one and the same, on account of this product, or should we rather consider them as distinct, and the microscopical appearances not peculiar, further than being the result, as in the case of the giant-cells of morbid change?

If, however, we accept Koch's theory without reserve, we must look upon everything as tuberculous in which the bacillus is found. By those who have investigated this subject, the acceptance of the theory has not generally been difficult. Indeed, no thorough work has been done to refute the experiments of Koch, but, on the contrary, all has tended to verify them. As the question stands at present then, and being forced into the opinion that, until such excellent work as Koch's is disproven by as good on the other side, we must consider the etiology of tuberculosis as discovered, I should be satisfied to place lupus among the tuberculous diseases if the bacillus is found in it.

In looking over the recent literature of the subject, the difference of the results obtained microscopically is striking.

Demme[1] found in three cases of lupus a few bacilli for the most part within the giant cells. The finding of the bacilli within the giant cells of lupus would suggest the possibility of finding them in a similar position in other diseases where giant cells are numerous. That Dr. Demme is possibly wrong in his observations, however, may be supposed from investigations made by Doutrelepont.[2] This author was not satisfied with his results. He found a few bacilli, but not enough to prove the tuberculous character of the disease. The number of bacilli varied extremely in different cases. They were present in large quantities in a specimen taken from lupus of the right cheek of a robust female. They were found most often intercellular, but not within the giant cells. In a preparation taken from the nose, there were seen, besides the bacilli of tuberculosis, some bacilli of another species which were stained. These latter were very numerous, and partly contained within the epithelial cells themselves.

I must call especial attention to the discovery of these last-mentioned bacilli, because it has a decided bearing upon what I shall say further on regarding the care with which such investigations must be made.

Pfeiffer[3] tried for some time in vain to demonstrate the tubercle bacilli in lupus taken from the conjunctiva. Being convinced that they must be present in it, he tried again, taking specimens from the conjunctiva of a girl, 11 years old, who had had the disease five years. The specimens were frozen, and coloured with fuchsin or methylene blue. In two preparations out of eight, bacilli were found; in one of these, their number was six or eight and in the other only two.

Malassez[4] did not find any bacilli in any specimen of lupus examined by him. Inoculations of fragments into the anterior chamber of the eye of rabbits also produced no result.

Cornil and Leloir[5] have made experiments by inoculating animals with lupus material. They insist upon the care to be observed in such experiments, and give four rules necessary to a satisfactory result: 1st, it should be certain that the animal to be inoculated is entirely free of tuberculosis; 2d, every antiseptic precaution should be taken; 3d, the

[1] Berl. klin. Wochenschrift, No. 15, 1883.

[2] Monatshefte für praktische Dermatologie, June, 1883.

[3] Berl. klin. Wochenschrift, July, 1883.

[4] Annales de Derm. et Syph., August, 1883.

[5] Société de Biologie, July, 1883.

animals to be experimented upon should not be placed in infected cages or near other tuberculous animals; 4th, the lupus should be taken before it has been treated, and when it is in an active state of development.

In ten experiments made according to these rules, all were negative excepting three. They state, as a result of their microscopical search for the bacillus, that if found in lupus at all it is "*extremely rare.*" In conclusion they call attention to the unsatisfactory result of their inoculations as compared to those made with true tuberculous material, and consider that lupus, if it is a local tuberculosis, is in every case a very attenuated one ("une tuberculose très attenuée").

A discussion arose following the report of these experiments in the Biological Society upon the question, whether, if this was the case, lupus could not be looked upon as an inoculation to prevent true tuberculosis. Cornil reported that experiments were being made to test this question, and it seemed to him possible that they might succeed.

Martin[1] does not consider the question that lupus is a local tuberculosis as settled. He maintains, however, from various experiments made by himself, that in a tuberculous subject, all the tissues of the body, no matter if they appear perfectly healthy after the most searching microscopical examination to prove the contrary, can, nevertheless, in a certain number of cases transmit tuberculous virus by inoculation. To prove this he cites many experiments, and among others the following: A fœtus was obtained a few hours after the abortion of a tuberculous woman (her tuberculous condition being proven by an autopsy several days later), which seemed perfectly healthy in all its organs, no lesion of the smallest size being found. The thorax was opened with a bistoury, heated previously red hot, and a small fragment of tissue was cut from the centre of one of the lobes of a lung. It was introduced under every antiseptic precaution into the peritoneal cavity of a Guinea-pig. Four months and a half later the animal died, and the autopsy revealed general tuberculosis. This experiment was prolonged by inoculation through four generations, with the result of producing tuberculosis in every case.

It has been shown by Cohnheim, Schüller, Heuter, Councilman, and others that non-tuberculous matter introduced into the peritoneal cavity does not produce tuberculosis. The fallacy in previous experiments, which were supposed to prove that tubercle could be produced in animals by any irritating substance introduced, lay in the fact that the animals thus experimented upon were kept in cages or laboratories teeming with tuberculous infection, and therefore their dying of tuberculosis proved nothing.

The bearing of Dr. Martin's experiments upon the subject of inoculation is evident, and they serve to convince one of the importance of casting out all chance of error before coming to any definite conclusion.

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[1] Ann. de Derm. et Syph., Nov. 1883.

Demme,[1] besides finding tubercle bacilli in lupus, reports having seen them also in the exudation from ozena occurring in a young child who had been exposed to tuberculosis in the house of a man affected with the same. The child died of tubercular meningitis, and the author considered it caused by infection.

In considering the various results cited, their difference can be explained in a great measure by supposing that the mechanical manipulation of the material to be examined had not been as carefully carried out as it should have been. In my own investigations, which were made in Prague, and which range over a series of ten cases, and in those made at the same time and in the same place by Dr. Symington of New York, which comprise at least fifteen cases more, we were never able to find any tubercle bacilli. Our method of staining was based entirely upon the theory which Koch lays so much stress upon, and which is the foundation of all his tests, viz., that the tubercle bacillus has a peculiarity of its own with regard to discoloration by an acid. While acid of the strength of $33\frac{1}{3}$ per cent. will entirely wash out fuchsin or methylene blue from the tissues, and other bacilli or cocci, it does not affect the colouring of the tubercle bacillus. In the field of the microscope nothing should be stained but the bacilli. They come out distinctly in contrast to the whiteness of the rest of the field, and are then as easily distinguished as any other microscopic object. When the rest of the field is not discoloured in this manner, no dependence is to be placed upon the examination. The whole truth of the investigation depends upon a knowledge of this fact, and the non-observance of it is very probably the reason why those above quoted differ so decidedly. Doutrelepont, as before mentioned, describes other bacilli in his preparations than those of tubercle. If Koch be correct in his statement that only the tubercle bacilli remain coloured after being washed out in strong acid, then this author's preparations must have been improperly manipulated, or there would have been no bacilli in them excepting those belonging to tuberculosis. Improperly prepared specimens of almost anything, as I have found by experience, will disclose any number of bacteria.

The specimens of lupus examined by me were taken from patients of different ages, and from different parts of the body. They were all patients of Prof. Pick in Prague, and for the most part cases of long standing which had broken out afresh. Sections were made for the microscope from fresh frozen specimens, as well as from those hardened in alcohol. Ehrlich's method of staining, which I consider the best of any yet proposed, was used. After they were stained, the sections were placed in the $33\frac{1}{3}$ per cent. nitric acid, and allowed to remain in it until they looked colourless; sometimes this was as long as five minutes. The same procedure with tuberculous material always disclosed bacilli, but I was not able to find any in the lupus sections examined. The necessity of discolouring so completely was definitely impressed upon my mind by a certain specimen of sputa which was examined in the same laboratory, and which, from the want of the above precaution, was pronounced to be tuberculous, but which the post-mortem examination did not substantiate.

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[1] Loc. cit.

My first two specimens were taken from the mucous membrane of the nose of a woman distinctly marked with lupus vulgaris on the face, the disease having extended thence into the nose. Sections were made of pieces taken from each nostril, both frozen and hardened in alcohol. In none of them were bacilli found.

In the next two cases, scrapings of lupus vulgaris from a boy's leg, which were hardened in alcohol, were twice examined with a negative result.

The other six cases were from different persons, three of them being taken from the mucous membrane of the nose, where the disease was supposed to have made its latest invasion. No bacilli were found in any of the sections.

The fifteen cases examined by Dr. Symington at the same time I cannot report fully upon, but the result was the same, as no tubercle bacilli were found in any of them.

From the searching examination which all these cases were put through with by persons who were really anxious to find them, I feel quite sure that no proper tubercle bacilli, such as Koch describes, could have missed discovery. The finding of a few bacilli in a large number of sections is really a negative result, or an accident, for, if lupus is a form of tuberculosis, and if we consider the bacilli the cause of it, they should be present in such a disease as lupus where there is unmistakably an active regeneration taking place in sufficient quantities to allow of no doubt.

Reasoning upon these investigations, which are to a great extent upheld by those of others, one is forced to the conclusion that the result is negative, *i. e.*, the presence of tubercle bacilli in lupus has not been satisfactorily proven. In many minds such a result would be sufficient to prove the non-identity of the two diseases, but for those who do not believe so implicitly in the bacillus as that would require, the question is still an open one. We must refer them to the experiments made upon animals by inoculation. In these there need be no question of a bacillus as the cause of the disease. The material inoculated may contain any unknown virus, but should, whatever this may be, produce the specific disease. This lupus does not do with sufficient promptness to warrant us in classing it as tuberculous, and although Cornil suggests that it may be a kind of attenuated tuberculosis, yet the clinical fact that those affected with lupus are not necessarily exempt from tuberculosis, makes one sceptical, *à priori*, as to the success of their proposed experiments.